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WKWK

WHAT KNOWLEDGE IS WORTH KNOWING

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The very title sounds arrogant; who am I to tell anyone what they should or should not learn?

The problem is awkward but it will not go away. First, it is awkward because who can pretend to know the future at all accurately? Second, to a fair extent the knowledge one can learn is a zero sum game; time spent learning one thing is not available to learn a different topic. Third, the problem is of increasing importance since knowledge is doubling approximately every 17 years. The number 17 is not exact, but then neither is the definition of "knowledge". The growth rate can be observed in the growth of the size of libraries, in the number of publications, and in the past size of research laboratories. Fourth, there is also the problem of obsolescence of knowledge: it has been estimated that half of what you learn in school is obsolete in 15 years.

In practice people tend to learn what ever happens at the moment to interest them, plus a large random, chance component; usually there is no planned, global approach to the question of whether or not they should learn something. This essay is an effort to get people to apply some of their intelligence to this critical problem for their career and not simply to drift and let what ever happens happen - which typically means a much less than optimal career.

The first point to be developed is that the future is much more subject to large scale, unforeseen changes than the average person thinks, and it is a reasonable conjecture that there at least a 90% chance that in your lifetime your field will have a rather complete upheaval of some of its basics. To see why this is true you have only to think of the following technical discoveries and their effects in many fields: transistors, fiber optics, continental drift, carbon dating of relics, radiation as used in medicine, computers, the discovery and exploitation of D.N.A., and even modern warfare with its exotic weapons. Then there are the immediate consequences of transistors such as the inevitable conversion from analog signals to digital signals that is going on right now throughout our whole society. The development of metalurgy has also so greatly changed what we can do and has had very large effects in many areas. Aviation is a combination of both the possibility of flight and the interaction with many other things to produce the great shrinking of the globe, as they say.

Second, that knowledge is almost a zero sum game is evident to most practicing scientists and engineers since there is more to learn than there is time and energy to learn, and at every turn there is a choice to be made. Most people make their choices almost at random which therefore are local choices, not global, and they suffer the consequences. You need to have a reasonable grasp on the problem of what to learn, and to use your intelligence to guide you through your lifetime career, rather than let chance control much of it. Hence the importance of this talk.

Third, since Newton's time (around 1700) we have coped with this doubling of knowledge every 17 years mainly by specialization. As we are now headed, the expert in the near future will know everything about nothing, while the generalist will know nothing about everything. Both paths are unsuitable. Awkward as the problem is, every teacher at every class period, as well as every student, constantly faces the problem of what knowledge is worth learning; but we usually prefer to ignore and not discuss it. I claim that the problem is now too important to continue to ignore. Just as for the population growth problem, it appears that if nothing is done then disaster also awaits us in the intellectual world. Computers will not save us in this matter.

A common explanation for the growth of publication is that the policy of the Universities, "publish or perish", produces the pollution of the streams of knowledge. This could be easily rectified if they would adopt a policy of considering for promotion only the best three, or five, publications, and look at quality rather than quantity of publication. Easy as it would be to partially cure this aspect of the problem, it is very unlikely that the Universities will do this in the near future.

There is also, of course, the problem that you must learn enough of the current material so that you can get a job and be useful, and hence get to your future in the field. The paradox is that in 15 years about 50% of the current knowledge will be obsolete and you cannot afford to concentrate on it exclusively, yet you must master it enough to get along now. The methods of "how to do things" seem to have a longer half life than the detailed facts of a field. Thus learning to derive things and developing your own creative talents is vital, and they need to be encouraged. Unfortunately most courses are taught by "these are the facts" approach and you are not encouraged to develop your own problem solving talents. Hence, you need to take an active part in your education and make the material your own by mastering it in your own way; most of your professors will not take make the effort to develop your problem solving abilities. More than you believe, your future is in your own hands, but it takes an extra effort on your part to arrive where you want to be in the long run.

In discussing the question of what knowledge is worth knowing we find that two words are regularly used, "fundamentals" and "basics", which seem to mean about the same thing. But it is useless to say these words without at the same time giving a method

for recognizing them. The method need not be perfect, but it ought to be reasonably effective in the long run. We need to recognize that it is not only the facts but the methods that are the basis of a field of knowledge; indeed, as noted above, the methods in the long run may be the more important part!

One approach to the fundamentals is to study history, and see what has proved to be long lasting. For example, in classical mechanics, Newton's three laws have stood the test of time even if the relativity theories and quantum mechanics have displaced them in some situations. Newton's three laws have been augmented by derivations from them to more useful forms such as the Lagrangean and Hamiltonian forms. But history is a dangerous model. When I went to Bell Telephone Laboratories in 1946 they were in the process of converting from relays to electronics, and I thought I ought to know something about vacuum tubes. So I both studied them a bit and privately assembled a few Heath kits. But I also helped, via computation on computers, the inventors of the transistor as well as other people in the transistor development process to go from the ideas to production. So much for my study of vacuum tubes! Fields of knowledge do become obsolete all too rapidly, and the experts are usually left behind since they are all too often hostile to learning the competing field's knowledge. As noted before, it is often said that the half life of the technical knowledge that is taught in school is 15 years, either due to replacement by newer ideas or by becoming obsolete because we have gone in another direction - like vacuum tubes and buggy whips.

Another approach to the fundamentals of a field is to see how small a basis can be used, along with fairly well understood methods, so that the rest of the field can be derived fairly easily. This, again, is not a certainly safe path; in signal processing the classical approach has been via the Fourier representation, (a global approach) but it may in time be displaced by wavelets, (a more local approach), or again it may not. Who can reliably say at this moment? Can we afford to retain both approaches with the corresponding cost?

The main defense I offer you to cope with the problem of "What knowledge is worth knowing?" is learning to learn. Since in your career there will probably be many new things that were not taught you when you were in school, it will be necessary for you to learn new things - often on your own, though increasingly these days there are seminars on the new topics. To remain an expert in your field it will be necessary for you to learn new things constantly. The pace of progress seems to be accelerating.

But learning to learn is not enough; you must also establish in yourself the habit of learning. All too often one sees people who could learn new things but they do not seem to do so, and hence they are left behind as their field progresses and changes.

It is generally seems easier to learn from others than it is to learn for yourself. But my life time study of creative people shows that what you learn from others you can use to follow, but what you learn for yourself you can use to lead. That does not mean that you should not go to seminars in new fields, but it does mean that you will need, in your own immediate area, to do a lot of self-teaching. And in all cases you should avoid passive and adopt active learning.

There is yet another matter to talk about, namely your personal style. It is obvious that if you are to do important things then you must work on important problems - it is extremely unlikely that working on trivial things will turn up an important result, though a lot of people like to think it will so they can justify their current actions. You need to work on the right problem at the right time and in the right way; nothing else will do. Success is not just a matter of luck since the fact is that the same famous people did many great things, as Einstein did when he wrote 5 classic papers in one year. Thus it is not just the possession of knowledge that matters, it is "style" that counts. You need to develop a personal style - doing things the way everyone else does will seldom lead to great new results - it is your peculiar way of looking at things that often is the key to success. Thus in learning new things you need to "digest" the ideas and make them your own, to integrate what you are learning into your other knowledge, to form interconnections, "hooks" if you wish, between the various parts of your knowledge.

In examining the fundamentals of a field it is the methods of deriving the results as well as what they are that matters. Drifting along, as most people do, learning locally what seems to be needed without any global planning for their lifetime, is not likely to succeed in this highly competitive world.

Besides encouraging you to develop yourself, all I can really do in the time available is to give you two examples of how I approached two, new to me, fields of knowledge, and suggest that you similarly need to find your own approaches - copying me will not be enough.

I will omit how the situation arose, but the end of a conversation in a hallway with my then Vice President at Bell Telephone Laboratories about the need, right then (about 1973), for someone to write an elementary introductory text book to the topic of digital filters he said, "Yes, Hamming, you should.", and walked off! Not only did I know comparatively little about the topic, but in fact I rather disliked it! I tried to get others, more capable than I was, to write the book, but in the end I had to do it myself. The arrangement I soon made was that a friend, J. F. Kaiser, would go to lunch with me regularly in the company restaurant (and at other times, too) he would educate me on the topic.

I knew that the classical approach to signal processing was to

represent the signal by either a Fourier series or a Fourier integral. Being a mathematician I immediately asked, "Why those functions?", since any complete sets of functions, say the Bessel functions, would do just as good a job at representing continuous functions with only a finite number of discontinuities. The replies I got from various electrical engineers were unsatisfactory to say the least, and some were downright wrong. I had to extract from my own knowledge the following facts: (1) the Fourier representation is time invariant, meaning that a translation in time does not change the quality of the fit, though it changes the coefficients in a known manner, hence the Fourier functions are the eigenfunctions of translation; (2) they are also the eigenfunctions for linear systems, which the formula for a digital filter is; and finally, (3) I found for myself, that sampling a pure frequency at equally spaced intervals, and then from the samples reconstructing the function as best you could meant that you got a single frequency, possibly the original one or possibly one that was of higher frequency and which by Nyquist's sampling formula was the alias of a pure frequency. But that the Fourier functions were the eigenfunctions was not said to me by any engineer I asked, and I asked a large number of them! Thus I reduced the problem to one that is widely familiar and has an extensive literature to draw on. Incidentally, I have never been able to get an electrical engineer to volunteer the remark that the transfer function, which plays so large a role in signal processing, is merely the curve of the eigenvalues.

I soon came to the realization that their basic approach of transferring the analog filter theory to the digital filter theory was foolish, though of course at times it could provide useful suggestions. I saw clearly that digital filters was an essentially new field and not an extension of the classical analog field. This is not an easy thing to do for a beginner, and all too often the elder statesmen do not recognize a new thing when they see it, rather they try to cram it somehow into their old beliefs.

Another major step in the process was that once, after telling me many different ways of designing a digital filter, Kaiser drew on the back of the dining room placemat a general method. I asked if this would do as well as the special methods, and he had to agreed. Why, then, all the special, trick methods? No real use at all! I left them out of the book.

The main criterion I used at every step was: "How little need one know to get along, not how much can I pile on the reader?" Another criterion was: "Not what is traditional, but what will be needed in the future?" Thus I felt I had to include as digital filters both differentiators and integrators. The last runs against the rule from old analog days, "A stable filter for a bounded input has a bounded output." But an integrator which takes in a constant must deliver an output that grows linearly with time, hence is unstable by their criterion. I concluded that they had the wrong definition for stable digital filters, that "stable" should mean "not exponential growth" rather than "bounded output".

They still use the old definition that applied to analog filters!

How few ideas did I really need to cover the material? How little of the applications did I need to include? How well could I relate things to what they already knew? How few techniques of mathematics did I really need, not how many could I show off using? Some things, like the Gibbs' phenomenon are central and hence I had to master it; in the process I realized that if I knew how to mitigate the Gibbs' phenomenon for filters, then I knew how to do it for telescopes, and hence for electron microscopes. The last I promptly pointed out to friend who operated one - and he adapted the ideas to his electron microscope! Fundamentals go a long way!

There are problems when writing a book of selecting and presenting the material, and I included one fairly useless method of filter design because it was the best way I knew to present some later ideas that were essential. There was also the problem of getting somewhat near to current practice as well as laying down things that may be needed for understanding further developments. These are not easy tasks to accomplish in a small book, (it had to be fairly small if it was to be read by those for whom it was designed).

While writing the book I arranged to give a short one week summer course at UCLA, and I also tried out the notes on a few friends to test the presentations, and to find errors. The book finally went through three editions!

The second example of how I grappled with the acquisition of new knowledge is the development of the fiber optics. In this case I had only to keep abreast of things, and not do much beyond knowing enough to be able to supply the proper computations to the workers in the field.

One day, in the earliest days of optical fibers, I noticed that a talk on optical fibers was to be given that week. I realized immediately that optical fibers would naturally have a much greater bandwidth due to the frequencies of optical vs. electrical signals, and I also recalled that Alexander Graham Bell had once shown that the human voice could be sent over a light beam. Bandwidth is the basis of both volume of signalling and speed of pulses, not in velocity, but in compactness of the pulses, hence the speed of computers was included. I had to go and listen!

I believe it was in the first talk that the speaker remarked that, "God loved sand, He made so much of it." I realized that the speaker was implying that the current exploiting of marginal copper mines could be replaced easily when you used glass fibers rather than copper wires. No small point!

Having earlier assembled a number of Heath kits to learn about electronics and vacuum tubes I realized how much soldering went on; hence I promptly asked myself about the splicing of these hair sized glass fibers - if that could not be done easily then all the

rest would have a permanent difficulty. I focused on that as an essential step, though I also watched as at first the people in our Laboratories got clearer and clearer glass, and then the glass makers themselves entered into the task and finally produced glasses so clear, so they say, that if the oceans were that clear you could see to the bottom!

I avoided taking sides on the single vs. multimode arguments, though I had to be aware of it since I did computations for both sides. I felt that the single mode would win out for about the same reasons as the binary system dominates the computing field - and apparently I was right in that guess. I tried to think of the implications of optical transmission, and I supposed that later there would be optical computers. The much greater bandwidth could surely be translated into much faster computers, though it would clearly take time. Optical switching, when developed by the telephone companies for central office switching, would mean, I thought, much cheaper switching, and that would, as did the decrease in storage costs earlier, greatly affect both the design and use of computers.

I did not ignore the optical fiber developments, but I did not try to master them and become directly involved. I thought about their implications for my own field of activity. This, then, is an example of keeping up with a new field that will probably impinge on yours in time, without trying to gain a real mastery of it. This is a situation you will face many times, and if you are the average person you will do nothing and hence be surprised at the later developments in your field rather than anticipating them.

I have just given you two examples of what I did in situations you will increasingly face in your future: (1) situations in which you must master the new ideas, and (2) situations in which it will suffice to merely keep actively abreast. A very effective way to master a subject is to arrange to teach such a course, perhaps at night, at some local school. Usually they are only too glad to let you teach such a course; they save a lot of money in the process! Even better, if you want to really master a field, is to write a corresponding text book while you are teaching such courses. You really learn when you have to give serious consideration to what is and what is not essential, what order to present things, what kinds of proofs, examples, etc. you will use.

Merely keeping abreast of developments in your own field means that you will regularly be surprised by others making new developments instead of participating in them yourself. You must aggressively think of what lies in the near and far futures.

This leads into the important topic of communication. It is essential that you master this art. There are three aspects:

- (1) formal writing of books, reports, memos, etc.
- (2) formal talks to large groups, and

(3) informal, on the spot, presentations as the opportune moment arises.

This last point is not trivial - I have seen many "back room scientists" who could not respond when the decision was being made, but could only write a memo three weeks later as to why something else might be better. That is generally useless!

If you cannot communicate effectively then most of what you do will be wasted! It is as simple as that! Especially at the higher levels of an organization the word of mouth is one of the main communication channels, and long memos are almost always not read.

To improve your communication skills, you have merely to actively and regularly critique what you read and hear, and ask yourself carefully why you think it was or was not effective. Checking your opinions with that of others is, of course, necessary. You must develop your own style. There are thousands of books on "communication", and reading one or more would not hurt, but until you internalize the need, they will in general be ineffective. Like so many arts, it is a matter of you creatively doing the thing, not slavishly trying to copy others. When I found myself poor at public presentations to large audiences I deliberately sought out situations so I could practice and learn and hence I would not be a permanent cripple in this vital matter; I realized that, painful as it might be, I had to master that phase of communication, as well as the two other ones.

Not only are the number of publications increasing rapidly, but so are the number of talks being given, and somehow, as in publication, you must do it so well that people will pay attention to your work while ignoring that of others. If you are to be listened to you must become much better than average in communicating what you do in science and engineering!

To summarize the talk, the problems of the growth of knowledge, the growth of the rate of publications, obsolescence of current knowledge, and the limitations of how much you can learn, all present a serious problem to the scientist and engineer. The two main points in my solution are that you must master learning to learn (and establishing in yourself the habit of learning new things), and the other is learning to communicate effectively. You need to work on both problems as well as keep up in your field from moment to moment. Just keeping up is almost a guarantee that some time you will be left almost completely behind!

The implications of all this for teaching are left for you to consider.